SHORTER NOTES

A Method of Obtaining Abundant Sporulation in Cultures of Macrosporium solani E. & M.—During the recent exercises held in connection with the dedication of the completed laboratory building and plant houses of the Brooklyn Botanic Garden the writer reported a method by which abundant sporulation may be obtained in pure cultures of *Macrosporium solani* E. & M. Since the full report will not be published for several months, this abstract of the paper is given.

The method described consists essentially in wounding vigorously growing cultures after they are two or three days old. The fungus is grown in Petri dishes on string bean agar or potato agar. After cultures have made a vigorous growth, the mycelium is wounded by scraping the colonies with a sterile scalpel. Although undisturbed cultures produce few or no spores, those properly wounded fruit profusely. The more thoroughly the wounding is done, the more abundant will be the sporulation in any given culture. Great numbers of conidophores arise from the cells of the radiating mycelial strands which have been injured by the scalpel. Each conidiophore bears a spore at its tip. Many thousands of spores may be obtained from a single culture which has received the wound stimulus. It is thought that this method may be of interest to those who work with other fungi that do not fruit readily on culture media.

L. O. Kunkel

Lamium amplexicaule in Colorado.—I have today (May 4) collected this species in a vacant lot in Boulder. The genus is new to our Colorado list.

T. D. A. Cockerell

REVIEWS

Fritsch's The Algal Ancestry of the Higher Plants.*

Dr. Fritsch, in his interesting discussion of "The Algal Ancestry of the Higher Plants," gives special attention to trying to corre-

* Fritsch, F. E. The Algal Ancestry of the Higher Plants. The New Phytologist 15: 233–250. *f. 1, 2.* 9 Ja 1917.

late the alternation of generations as now known among the algae with the alternation of generations as exhibited by the Bryophyta and Pteridophyta. Inasmuch as these higher groups have pure green chloroplasts, he, like most other botanical phylogenists, looks for their ancestors among the green algae, and, inasmuch as the spermatozoids in these higher groups are isokontan, he looks for these ancestors more particularly among the isokontan green algae. Though admitting that the so-called sporophytic phase may have arisen in different ways in different groups of plants, he seems inclined, on the whole, to favor the theory that the sporophytic and gametophytic phases are homologous, that is, that they have "arisen by a gradual differentiation from an indifferent generation bearing both asexual and sexual organs" rather than that they are antithetic, that is, "that the sporophyte is a new intercation in the life history, originating by a gradual elaboration of the zygote." Accordingly, with a little bias, perhaps, in favor of the homologous theory, his likely algal ancestor is conceived to display the following tendencies: "Differentiation of prostrate dorsiventral and radial upright systems, assertion of a main axis in the latter, and restriction of sexual organs to the prostrate portion and of asexual organs to the appendages of the upright system." In the genus *Myxonema* (*Stigeoclonium*) of the order Chaetophorales, he finds species with a thallus showing in various degrees a differentiation between a prostrate, attached, dorsiventral portion and an upright, essentially radial, portion. This genus, however, lacks one of the characters of his hypothetical ancestor in that there seems to be no restriction of the gametangia to the prostrate base and of zoösporangia to the erect filaments. But in two or more species of *Trentepohlia* (*Chroolepus*), representing another family of the Chaetophorales, he finds indications of such a segregation of the gametangia and zoösporangia, this segregation being correlated, he thinks, with the terrestrial rather than aquatic habitat of the species of *Trentepohlia*. He notes that in some cases the zoösporangia and gametangia are found on distinct, though similar individuals. "There are thus," he says, "all the necessary indications for the gradual differentiation of two alternating generations, of which the one bears the asexual organs on the upright system, and the other bears the sexual organs on the creeping base. Disappearance of the base in the former and of the upright system in the latter (both phenomena which are known to occur among the Chaetophorales) will give two different generations, resembling those of the Archegoniatae in all essential respects." * * * "Such an origin, of course, amounts to an homologous one, though presumably of a somewhat different kind to that in the minds of the adherents of the homologous theory."

To Dr. Fritsch's theory as here formulated, two possible criticisms suggest themselves. The first and probably less serious of these criticisms is that thus far there seems to be no experimental evidence of any tendency towards an alternation of generations in *Trentepohlia* or in any other member of the order Chaetophorales. If the divergence of the prostrate sexual part and the erect asexual part was accompanied by each of these parts reproducing itself and not the other, which seems equally plausible, *a priori*, the final result would manifestly be what systematists would call two independent species. But it is, of course, conceivable that such a segregation and divergence as this may have occurred more or less parallel with a movement that resulted in an alternation of generations.

The second and probably more serious criticism is that the theory seems to give insufficient consideration to the fact that the cell-nuclei of the so-called sporophyte in the Archegoniates have twice as many chromosomes as do those of the gametophyte. Now the diploid and haploid relation of the chromosomes in sporophyte and gametophyte in the Archegoniates is so easily and obviously associated with the fusion of two gametes and the halving of the resulting chromosome number that it is almost inconceivable that it should have come about in any other way. Any supposition that the cell-nuclei of the hypothetical generalized ancestor may have had 28, 29, 30, or possibly a variable and indefinite number of chromosomes and that in purely vegetative ways the cells of one generation came to have always 20 chromosomes while those of the other came always to have 40 chromosomes, would be too improbable and fantastic to consider. The diploid and haploid relation must have arisen in the first instance, it seems fair to say, either through the doubling of the original ancestral number in the fusion of two gametes or through the halving of the original number in sporogenesis. If the diploid condition arose through the fusion of two gametes, then any phase or generation continuing it would be an "antithetic" generation under the definition adopted by Dr. Fritsch. If, on the other hand, the haploid condition first arose through the halving of the original ancestral number, then any phase or generation continuing it would escape technical conformity with the definition of an "antithetic" generation, but would the relations of the two phases be really different? Would not the haploid gametophyte be "intercalated" instead of the diploid sporophyte? Probably Dr. Fritsch and other supporters of the homologous theory would reply that the gametophytic generation would not be in itself a *new* intercalation under these circumstances and that the only new thing about it would be its sudden change from a diploid to a haploid condition owing to a shifting of the reduction in chromosome-number from gametogenesis to sporogenesis. Dr. Fritsch, noting that the reduction in chromosome-number occurs in some algae at gametogenesis, in others at the first division of the fusion nucleus, and in others at sporogenesis, evidently regards this as a cytological character of no particular phylogenetic significance. And with the amount of evidence now at hand it seems just about as difficult to prove him wrong as it would be to prove him right!

The writer of the suggestive paper under consideration regards the origin of the almost wholly dependent sporophyte of the Bryophyta as different from that of the soon independent sporophyte of the Pteridophyta, calling the alternation in the former antithetic and that in the latter homologous or rather "pseudo-homologous"—a conclusion that may impress many of his readers as being somewhat forced in view of the marked morphological and physiological similarities of these two groups of sporophytes in the younger stages of their development.

In the case of the tetraspore-bearing red algae, whose diploid generation consists of two spore-bearing phases, the attached sporogenous filaments of the cystocarp and the free tetrasporic plant, Dr. Fritsch accepts the view of Dr. I. F. Lewis that the first of these represents an intercalated antithetic phase, while the second represents a phase strictly homologous with the sexual plant.

MARSHALL A. HOWE

Hybrid Origin of Oenothera Lamarckiana*

In this paper Davis reaches an approximate conclusion on the old question as to whether *Oenothera Lamarckiana* is of hybrid origin. The parents used were *O. franciscana* from California and *O. biennis* from Holland, which he assumes may have met in England, from where he believes de Vries's *Lamarckiana* came. The form obtained resembles *Lamarckiana* rather closely, but the assumption of the possibility of a cross between species native to regions as far apart as California and Holland makes the hybrid origin of *Lamarckiana* seem less convincing than if the assumed parents were found growing in closer proximity.

Davis calls his form *O. neo-Lamarckiana*. It is now in the fourth generation from the original cross and was derived from a single plant selfed in the F_2. From the "most promising" F_3, 549 offspring lived to be set out into the garden. Of these 198 resembled *Lamarckiana* de Vries, while the other 351 suggested *franciscana*. The author recognizes some variation among the neo-Lamarckiana plants, but he says "the best plants are so close to the *Lamarckiana* of de Vries that I can only distinguish them by small plus or minus expressions of a few characters." Davis does not state whether all the observed variations of his *Oenothera* fall within the range of variability for de Vries's *Lamarckiana*.

Davis tests the breeding behavior of *O. neo-Lamarckiana* with reference to the production of twin-hybrids and the throwing of mutants, which are the most important characters of the true *Lamarckiana*. He obtains twin-hybrids, but it is perhaps not at all established that the twin-hybrids of de Vries or Davis are

* Davis, B. M. Oenothera Neo-Lamarckiana, Hybrid of *O. Franciscana* Bartlett × *O. Biennis* Linnaeus. Am. Naturalist 50, 688–696, 1916.